Cross Currents in the History of Human Genetics

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THERE IS, I BELIEVE, general agreement that interest and activity in human genetics has today reached a peak never before attained. The periodical literature of the last ten years and the reports of the increasingly frequent symposia and conferences devoted to genetic problems in man provide convincing evidence of this. It is also clear that interest in these problems is likely to increase greatly in the next years so that what we may be witnessing now is only the beginning of a kind of renaissance in which genetics in general stands a chance of being greatly enriched by research on man.

There are probably many reasons for this rather sudden spread of interest, but I think that now is not the best time to try to identify the specific causes and influences of the change. For me, at any rate, a more interesting question is why this period has been so long delayed. Why did human genetics develop so slowly? It is, after all sixty years since the basic principle of heredity came to recognition. By 1915 the general architecture of the hereditary material was known (The Mechanism of Mendelian Heredity, by Morgan, Sturtevant, Muller and Bridges). Even if we date the definitive elucidation of the physical basis of heredity as late as from the publication of Morgan's *Theory of the Gene* in 1926, still that knowledge has been with us for 35 years.

I know the stock explanation of lack of progress used in those days when human geneticists were inclined to be apologetic: "You see, we can't experiment with man, and his generation time is long." True, no more can we do experimental breeding with him today, nor has his generation time decreased. Yet the rate of learning about human genetics has greatly increased today. Great progress has only recently been made in several fields in which essential steps opening them to investigation were taken long ago. The primary generalization of population genetics was adumbrated by Pearson in 1904 and clearly formulated by Hardy and by Weinberg in 1908, and its usefulness in human genetics demonstrated by Bernstein in 1924. By 1930 the groundwork of general theory in this field had been laid by Haldane, Fisher, Wright and others, but there has been a long lag period in the application of such methods to man. Many of the implications of Mendelian genetics for studying the transmission system, gene action, bio-chemical genetics and evolution in man were foreseen by Garrod in 1908 and some of them even in 1902 by Bateson. Yet they too have

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only recently been exploited in human genetics. Cytological study had, even in the 1920's, facilitated the resolution of genetical problems in other animals and in plants yet did not begin to serve this function in human genetics until the mid-fifties.

I do not mean to say that new technical and analytical methods have not had important effects in facilitating progress. They certainly have. I do mean to say that methods and ideas already available were not, for many years, applied vigorously and with good results to the study of human genetics.

I have recently been re-examining the history of genetics in the formative period from 1900 to about 1930. I have gained the impression that influences which played on human genetics during that period had a good deal to do with delaying its progress in the next 20 years and have not yet ceased to operate. It was then caught up in the crosscurrents to which all studies of man are exposed. The effects of science on human life are always immanent, yet never so immediately apparent as when man himself is the object of inquiry. In the period of which I speak, his confidence as controller of his own destiny had been aroused by recent scientific discovery and by social and political conquests of new environments. Rapid translations of new knowledge into terms applicable to improvements of man's lot is at such times likely to take precedence over objective and skeptical evaluation of the facts, a danger of which cautious scientists have long been aware. The testing of hypotheses by factual observations and the construction of general theory, the normal methods of science, are certainly no less important when human beings are involved, yet one often finds these neglected in human genetics in that period. Progress in human genetics seemed to have been impeded less by lack of means than by lack of a clear scientific goal, and this at a time when the major problems of genetics were taking a clear form. The particulate nature of the transmission mechanism of heredity had focused attention on the means by which genetic elements reproduce and maintain their continuity with opportunity for change and evolution, and on the means by which genes control metabolism and development. But most observations on human heredity were not oriented in any clear way toward such problems. Matters of greater moment seemed to be the inheritance of "insanity," of "feeblemindedness" and other then vaguely defined mental ills, the effects of parental age or alcoholism or social status on the offspring, and similar studies pursued for immediate social ends.

An interesting comparison, which I shall not be able to pursue in detail, is that between such dominant interests of the period as those just cited, and the direction initiated by Garrod's paper of 1901 (Lancet, Nov. 30) on alcaptonuria and especially by his Croonian lectures of 1908. One reads today those lectures as published in successive issues of Lancet for that year with admiration for the depth and breadth of Garrod's scientific understanding of genetics and of evolution, and then turns with amazement to the reports of discussions on human heredity at the Royal Academy of Medicine which ran through five later issues of that same volume. Except for Garrod's strong supporter and genetical advisor, William Bateson, there is little evidence that the numerous participants in those debates realized what, in fact, the problems were. Karl Pearson, the director of the Galton Laboratory of National Eugenics, was reported to have

said in the third debate: "His own view was that there was no truth in Mendelism at all." (Lancet 2, p. 1615). He insisted that he had been misreported, although two independent records confirmed the quoted statement (p. 1768), and that he had said that "Mendelism had not been demonstrated for any one character." (Lancet 2, p. 1708). But the main lesson we learn from the above is that Garrod's work had little effect until many years had passed; while those interested in the social applications proposed by eugenics largely dominated the field of human genetics.

It will, I think, be clear to anyone who examines the records of the period from 1900 to about the middle thirties that the manner in which the eugenics movement developed cast a long shadow over the growth of sound knowledge of human genetics. The ideals of eugenics as originally proposed by Galton in 1883 and restated in more concrete form in 1901 can hardly be held responsible for this, for they will appeal to most people as embodying a noble conception. But there grew up within the eugenics movement ambivalent attitudes through which it tended to become all things to all people, here a science, there a social movement, and in Germany an instrument, though the so-called eugenics laws of 1933, of the ferocious application of prejudice which seemed to many people to be the logical extension, the reductio ad absurdum, of ideas to be found in eugenic programs elsewhere. One effect of all this was to deflect attention from the essential scientific problems and to discourage persons interested in these from pursuing them with human material. It seems as though some perverse kind of Gresham's law might have been operating here, bad coin driving out the better.

A second cause of failure and delay in human genetics was the all too frequent relaxation of critical criteria and a lowering of standards which would not be tolerated in other branches of genetics. In course of time this, like the handicap imposed by eugenics, became less important in relation to the rising tide of good scientific work, both practical and theoretical, in human genetics. Signs of the change may be seen in Penrose's paper of 1932 and in Hogben's book of 1931 which contained a sharp attack on eugenics. As these changes went on, the name eugenics disappeared from several institutions and publications dealing with human genetics. On the other side some of the eugenical organizations (like the American Eugenics Society) tended to assume a more responsible attitude toward the scientific facts underlying social applications and toward research in human genetics.

I think it can be shown, however, that neither of the chief defects seen in the adolescent period of human genetics has in fact disappeared today. Now while I suppose that the chief function of historical analysis is to gain views that are more satisfying, intellectually and esthetically, than those afforded by studying only the present state of knowledge, still it has its practical side as well, since we can hardly overlook lessons for the conduct of our lives in the present and the future.

In both of these respects the history of connections between eugenics and human genetics has a special relevance. The connections were very close, and were especially evident in the United States, where interest in both fields was widespread at the turn of the century. Human genetics was often treated

as part of eugenics, or as it was often called, human betterment or race improvement. It was that part concerned with acquisition of knowledge of human heredity. The association tended to be maintained because both subjects were frequently pursued and often taught by the same persons. Those who had been attracted by the promise inherent in the newly discovered work of Mendel often added to their repertoire the results of earlier studies like those of Dugdale (1874), and others who had dealt with mental deficiency and criminality as social problems.

There were, however, a few whose position was most clearly stated later by Bateson (1919). "The eugenist and the geneticist will, I am convinced, work most effectively without organic connection, and though we have much in common, should not be brigaded together. Genetics are not concerned with the betterment of the human race but with a problem in pure physiology, and I am a little afraid that the distinctness of our aims may be obscured." But in general the position in most countries was that implied by an index entry in the one serious attempt to trace, from documents, the history of some of the important ideas about heredity. That was the essay of Alfred Barthelmess (1952) in which under the entry *Mensch* we find "sieh auch Eugenik."

The nature of the relations between eugenics and the study of human heredity was strongly influenced by three facts. The first is that the formulation of the problems and program of eugenics antedated the recognition of the particulate nature of heredity. Early work in eugenics was thus guided by a view of heredity which proved to be without general validity. The second is that eugenics achieved organized forms before genetics did. It thus became at the very least a part of the environment in which genetics grew up. The third is that stated by Bateson: they had different goals.

The development of my argument now requires a brief sketch of the history of eugenics. The best source book for this is Karl Pearson's great four volume biography of Galton (1914-1930).

We may formally date the beginnings of eugenics in its modern form from Galton's Huxley Lecture to the Royal Anthropological Institute (published in Nature, Nov. 1, 1901) on "The possible improvement of the human breed under existing conditions of law and sentiment." Galton's ideas on this subject had been adumbrated long before this time, first in a paper of 1865. By 1883 they had been given the name of eugenics but had not then attracted active attention. Nor did the seed sown before the anthropologists in 1901 appear to have taken root quickly in England (although it fared better in the United States) and it took further effort on Galton's part to get a fellowship in eugenics established at University College, London, in 1904. This led first to the organization of a Eugenics Record Office and then in 1907 to the Galton Laboratory of National Eugenics (both endowed by Galton) and in the next year to the organization of the Eugenics Education Society. One should note two coincidences of date: 1865 was also the year in which Mendel presented his results at Brünn; and 1900 was the year of the famous "rediscovery" of these. But there was no more connection between the ideas of Galton and Mendel in 1900-01 than there had been in 1865. It was chance of the same kind which gave both men the same year of birth—1822.

Galton's ideas concerning eugenics had been formed first after reading the Origin of Species. The substitution of social control for natural selection in guiding human evolution was for Galton "the logical application of the doctrine of evolution to the human race," but the first ambivalence appeared when he added that the result of his study had been "to elicit the religious significance of the doctrine of evolution. It suggests an alteration in our mental attitudes and imposes a new moral duty." He had become convinced of the heritability of mental qualities through his studies first of Hereditary Genius (1869) and then of Inquiries into Human Faculty (1883), and had devised statistical methods for the study of inheritance which led him to his Law of Ancestral Heredity in 1897. His views on heredity were always based on this "law" which turned out to describe certain resemblances in graded or continuous characters between parents and offspring but of course provided no explanatory or general principle such as that discovered by Mendel. This is not to say that his eugenical proposals would have been invalidated by his acceptance of Mendel's principles. Those proposals were based primarily on the supposition that heredity was an important cause of differences in physical, mental, and moral qualities, and that was sufficient for his purposes.

Divergences soon appeared in England both among the supporters of eugenics and between these and the school which was shortly to call itself genetics, but was at first referred to as Mendelians. The internal cleavage in eugenics was that between the research and the propaganda interests, as represented by the Eugenics Laboratory and the Eugenics Education Society. The Laboratory resisted and resented interference with its primary function by the Society. "It will never do," wrote Galton to Karl Pearson (the director of the Eugenics Laboratory) on February 6, 1909, "to allow the Eugenics Education Society to anticipate and utilize the Eugenics Laboratory publications" (Life Vol. IIIa, p. 371), and he reminded the Society of the "differences between the work of the two classes of publication." The founder of the movement saw quite clearly the distinction between research and propaganda, and in his last public lecture on "Probability, the Basis of Eugenics" (Oxford 1909) he came out for research as the immediate need, social application as the distant goal.

But dissension between Society and Laboratory continued and finally Galton was impelled in a letter to the London Times (Nov. 3, 1910) to make his position quite clear vis-a-vis the Eugenics Laboratory and the Eugenics Education Society. "Permit me," he wrote, "as the founder of one and the honorary president of the other, to say that there is no other connection between them. . . . The Laboratory investigates without bias . . . large collections of such data as may throw light on many problems of eugenics. The business of the Society is to popularize the results." (Life, Vol. IIIa, p. 408). This cleavage, which was to reappear time and again as the movement grew, marked a separation, often not well defined, between those interested in science and those interested in social and political questions. The progress of genetics may not have been directly affected by such disagreements within the eugenics movement, but the occasional excesses of persons with political motivation revealed the source of danger which eventually broke into the open in Germany.

The other cleavage which became apparent at once in England was that

between the Mendelians led by Bateson, and the followers of Galton, led first by Weldon and then by Karl Pearson, and known as the Biometricians. The verbal battles between these sharply opposed schools certainly did delay the development of both genetics and eugenics in Great Britain. Karl Pearson, the first director of the Eugenics Laboratory, and, after Galton, the leading eugenist, never recognized the importance of Mendel's principles upon which genetics was founded. As late as 1930 he could say (Life of Galton, Vol. III, p. 309) "during the last 25 years we seem scarcely nearer the exact knowledge of the laws of heredity; the farther we advance the more complex does the problem become." It was not that he (or Galton) failed to understand the primary principle of segregation, although he did not appreciate the relation of dominance to it. Indeed in 1904 Pearson foreshadowed an important extension of the principle of segregation by showing that Mendel's ratio 1DD : 2DR : 1RR tends to maintain itself indefinitely in random breeding populations of large size (cf. Wright 1959). Galton likewise applied the term "atomistic" to Mendel's system; but neither Galton nor Pearson nor their followers found their interest satisfied by the new principles of Mendel. The heredity in which they were interested could not (they thought) be studied in that way. What they thought important to understand was quantitative variation in human intellectual ability, and Mendelism they considered to be of no help at all. In fact at that stage it was not helpful. This of course is only to say that the purposes of the biometricians and eugenists differed from those of the protogeneticists. Purposes, like tastes, may not be fair game for scientific dispute, although neither side admitted that.

In general the alienation between the two schools was a local British affair. One aspect of it however involved the beginnings of the eugenics movement in the United States. There the ground had been prepared by studies like those of Dugdale (1874) on the Jukes family and of Alexander Graham Bell (1883) on deaf mutism. However in name and purposes the eugenics movement in the U.S.A. was clearly descended from the British one. It differed sharply from its parent in its attitude toward Mendelism. The first proponents of eugenics in the U.S.A., of which C. B. Davenport was the most active, were thoroughgoing Mendelians, and eugenists because they were Mendelians. In fact, Davenport might have been called a super-Mendelian. One has only to read his conclusions on the monofactorial inheritance of a violent temper or a wandering habit to realize this. The British eugenists correctly surmised that this attitude could (as in fact it did) bring the whole movement into disrepute. Dr. David Heron of the Galton Laboratory vigorously attacked in 1913 the first papers to come from Davenport's newly established Eugenics Record Office (founded 1910). Heron wrote (p. 5): "We have selected this rounded group of papers because they deal with a very pressing subject, that of mental defect, and in our opinion form a very apt illustration of the points just referred to, i.e., careless presentation of data, inaccurate methods of analysis, irresponsible expression of conclusions and rapid change of opinion. . . . The Mendelian conclusions drawn have no justification whatever." And further (p. 61): "The authors have in our opinion done a disservice to knowledge, struck a blow at careful Mendelian research, and committed a serious offense

against the infant science of eugenics." Heron's criticism, it must be acknowledged, was more than merely another skirmish in the war being waged between the Biometricians and the Mendelians. In this case the point at issue was fundamental scientific method, and Davenport and his collaborators were at least guilty of a lack of caution from which the whole eugenics movement was to suffer. It was at this time, 1910-1915, that single gene interpretations began to be applied with great confidence (amounting in some cases to recklessness) to differences in mental ability and to mental diseases. The outstanding example was feeblemindedness, and on the basis of the first pedigrees published by Goddard in 1910 Davenport (1911 Eugenics Record Office Bulletin 1) adopted the hypothesis that mental deficiency in general was inherited as a Mendelian recessive. In this he was followed by many others, and eugenical programs and some legislation were based on this assumption. Stanley P. Davies, who reviewed the history of this period in 1923, called it "the alarmist period." The first fruits of new methods of mental testing were garnered rapidly and widely, and the overemphasis on bad heredity as the cause of mental deficiency and mental disease, and on restrictive or negative eugenics as the only possible cure of a social ill brought on its inevitable reaction. H. S. Jennings in 1925 attempted to restore common sense by his critical attack on the whole concept of unit characters and on the unreality of the either-or distinction between heredity and environment in the determination of human (or any other) characters. Raymond Pearl in 1928 said: "Orthodox eugenists are going contrary to the best established facts of genetical science and are in the long run doing their cause harm." One of the signs that the public image of eugenics had been affected by this and similar criticisms was revealed when G. K. Chesterton published in 1922 *Eugenics and Other Evils*. These essays are not the best example of Chesterton's wit and journalistic skill, but the main point was made sharply clear to his large audience. These essays, he said, had been accumulating since before the first World War, and he had thought the defeat of Germany would have rendered them obsolete. But, he said in his foreword: "It has gradually grown apparent, to my astounded gaze, that the ruling classes of England are still proceeding on the assumption that Prussia is a pattern for the whole world. For that reason, three years after the war with Prussia, I collect and publish these papers." The essence of his objections to eugenics is revealed in one sentence (p. 51): "Even if I were a eugenist I should not personally elect to waste my time locking up the feeble-minded. The people I should lock up would be the strong-minded." Although his criticisms were not always cogent, his suspicion of eugenics, race hygiene, and "scientific officialism" of the German type proved to have been well-founded.

In Germany the eugenics movement took the name Rassenhygiene from a book of that title published in 1895 by Alfred Ploetz who also founded in 1903 the chief German journal in this field, the Archiv für Rassen-und-Gesellschaftsbiologie. In an article in this journal in 1909 Galton agreed with the editor that Rassenhygiene and Eugenik were to be regarded as synonymous. Any misunderstanding on this score was removed in 1931 when the chief German society in this field, the Deutsche Gesellschaft für Rassenhygiene

(founded in 1902) added "Eugenik" to its title. The direction in which Rassenhygiene led had become evident long before Hitler came to power; and the advent of the new laws for sterilization of the unfit and unwanted, and for the exclusion of Jews from the new state were greeted with editorial acclaim in the Archiv. The speed with which the first of these laws were prepared and promulgated within the first few months of 1933 is probably to be explained by the composition of the committee of experts which drafted them. This included Ploetz and his fellow eugenists Rüdin and Lenz and others who had worked in this field together with Heinrich Himmler. Frick, Hitler's Minister of the Interior, whose department was charged with the administration of the laws said upon their coming into force: "The fate of race-hygiene, of the Third Reich and the German people will in the future be indissolubly bound together" (Arch. Rass. Ges. Biol. Vol. 27, p. 451). The situation was made quite clear by von Verschuer in the introduction to his book *Leitfaden der Rassenhygiene,* published in 1941.

"Es ist entscheidend für die Geschichte eines Volkes was der politische Führer von den Ergebnissen der Wissenschaft als wesentlich erkennt and zur Tat werden lässt. Die Geschichte unserer Wissenschaft ist aufs engste verknupft mit der deutschen Geschichte der jüngste Vergangenheit. Der Führer des deutschen Reiches ist der erste Staatsmann der die Erkenntnisse der Erbiologie und Rassenhygiene zu einem leitenden Prinzip in der Staatsführung gemacht hat." (p. 11)

(Decisive for the history of a people is what the political leader recognizes as essential in the results of science and puts into effect. The history of our science is most intimately connected with German history of the most recent past. The leader of the German state is the first statesman who has wrought the results of genetics and race hygiene into a directing principle of public policy.)

This statement by a leading German human geneticist was made with some deliberation, for it appeared first in identical form in an article by von Verschuer in Der Erbarzt 1937, p. 97, and although it has been omitted in a recent edition of the above book (1959), the author has not to my knowledge publicly altered his position on enforced race hygiene. Although not all geneticists who remained in Germany thus accepted the eugenical and racial doctrines and practices of the Nazis, there is at least evidence that even the serious scientists among them underrated the dangers of the movement until it was too late. From this the melancholy historical lesson can be drawn that the social and political misuse to which genetics applied to man is peculiarly subject is influenced not only by those who support such misuse, but also by those who fail to point out, as teachers, the distinctions between true and false science.

In von Verschuer's book of 1941 Galton is acknowledged to be the modern founder of race hygiene as eugenics; but to Gobineau is given the greater credit of having first brought race into politics, thereby becoming the founder of political anthropology, a field in which the leading later exponents in Germany were Eugen Fischer and H.A.K. Günther. In contrast to the situation in Great Britain in which Galton had been unable to arouse the interest of anthropologists, the German eugenics movement had close connections with the kind of anthropology which was pursued by anthropometric methods. Since this was not guided by a theoretical rationale such as might have been supplied

by population genetics, it fell the more quickly a victim to the pseudo-science of the promoters of the Aryan mythology. The chief research institute was the Kaiser Wilhelm Institute for Anthropology, Human Genetics, and Eugenics, of which Eugen Fischer was the director. Many members of this institute had become so politically involved with Nazism that after the defeat of Hitler's regime the institute was not continued by the West German State, thus fulfilling rather quickly Frick's prophecy of the interdependence of race hygiene and the Nazi state. It must be noted that in the debacle eugenics carried anthropology and human genetics down with it. There can be no doubt that in Germany, formerly a center of genetical research, the effect of its association with race hygiene was to delay for a generation the development of a science of human genetics.

In the United States, as in Britain, anthropologists in general did not respond to eugenical appeals. The kind of racialism which had become attached to eugenics was not calculated to appeal to persons whose profession it was to study and interpret human differences objectively and in socio-cultural as well as biological terms. Human genetics has today become a useful contributor to anthropology, mainly through gene frequency studies, and by the application of good objective methods generally untinged by racialism. However, there are still reminders of the uncritical use of what look like genetical methods applied to racial anthropology. What shall one say, for example, when three authors, after anthropometric examination of 44 Italian war orphans of whom the father was unknown but assumed to be "colored" draw sweeping conclusions concerning heterosis ("established with certainty"), inheritance of erythrocyte diameter ("very convincing") and other statements not supported by evidence. Yet these are statements made in 1960 by Luigi Gedda and his co-workers Serio and Mercuri in their recent book Meticciato di Guerra. R. R. Gates, who writes an introduction in English to this elaborate book refers to it as an important contribution to what he calls "racial genetics." Others will have greater difficulty in detecting any contribution to genetics, but may see in it, as I do, a reflection in 1960 of the uncritical naiveté of that early period of human genetics which delayed its progress. And the same year — 1960 — sees also the appearance of a new journal "Mankind Quarterly" devoted in part to racial anthropology of the above kind (again described as such by one of its editors—R. R. Gates) and embodying racist attitudes of the earlier period. Truly the past is not yet buried, and human genetics, in spite of its recent evidences of new life, is still exposed to old dangers.

Eugenics movements grew in many other countries in the period before and just following the first World War, but space will permit taking account of only one such development.

It may be regarded as an anomaly of history that in Russia eugenics did not appear in an organized form until after the revolution of 1917, and this notwithstanding the enunciation of ideas very similar to those of Galton by W. M. Florinsky in 1866. In 1919 a eugenics department was started in the Institute of Experimental Biology in Moscow under N. K. Koltzoff, and shortly thereafter a Eugenics Bureau began in Leningrad under J. A. Philiptschenko. By 1925 thirteen research articles on human genetics, *sensu stricto,* had been

published from these institutes. The Russian Eugenics Society was founded in Moscow in 1920, with local branches in Leningrad, Saratov, and Odessa, and the Russian Journal of Eugenics under Koltzoff and Philiptschenko began in 1923 (cf. Koltzoff, 1925). The difficulties and later suppression of eugenics in the Soviet Union were foreshadowed by an event in connection with the publication in 1924 of Philiptschenko's book on eugenics. While it was in production at the government printing office, according to Weissenberg (1926) there was inserted in the introduction a statement to the effect that measures with important eugenical effects were the destruction of the bourgeoisie and the victory of the workers. What part the existence of eugenics there played in bringing about the suppression of genetics in the USSR is not clear to me. The first institute to be suppressed appears to have been that concerned with human genetics, the Gorky Institute for Medical Genetics, but this may have been incidental to the condemnation and execution of its director, S. G. Levit, as a "traitor."

Although the chief crosscurrents operating on human genetics were generated by persons pressing for social and political regulation of human breeding, frequently to the neglect of sound scientific method, others of less marked and definite character traced to relations, or lack of them, between genetics and medical research. Apart from lack of understanding of genetics on the part of physicians, there were frequent expressions of active lack of interest, since principles discovered in peas and exploited and extended by experiments with flies were not thought relevant to human beings. And if, physicians often said, a disease was inherited, that meant it couldn't be treated and knowledge about it was not likely to be useful. The gap due to mutual lack of appreciation and of common experience and training as between medical men and geneticists has shown some signs of narrowing, but is certainly far from being bridged. This would require further will and effort on both sides.

Nor can one conclude as yet that the confusion between the aim of eugenics and the facts of human heredity which Bateson pointed out has yet been cleared up. As eminent an acknowledged leader in genetics as Professor H. J. Muller has recently restated the adherence to ideas on controlled human breeding which he outlined in his book of 1935 *Out of the Night*. The central idea, eutelegenesis, had been developed by Herbert Brewer (1935) who probably was unaware of an earlier similar proposal by A. S. Serebrovsky (1929). The essential feature of the proposal was to utilize the sperm of men, chosen on the basis of achievement as superior, and by increasing through the use of artificial insemination the numbers of offspring of such superior sires to raise the average level of ability of the next generation. In his reiteration of this proposal in 1959, Muller has refined and extended it. He now proposes to retain the whole genotype of such men (which the processes of meiosis would tend to break down) by multiplying samples of their diploid spermatogonial cells in tissue cultures and subsequently obtaining embryos from these by some form of ectogenesis. Even though the technical problems involved might in some future time be solved, several more important scientific ones would still remain. Such schemes assume that there is an ideal genotype for a human being. Plato could entertain such an ideal but can we do so after

our experience of the variety of genotypes in successful populations? Can human cultures be maintained by an ideal genotype? Even though the proponents of eutelegenesis should admit that there might be several good kinds of human being, are there objective scientific criteria by which they might be selected? Even though choice of sperm or genotype donors were to rest with persons as benevolent and acute as I believe Professor Muller to be, selection would still be subject to changing tastes and ideals and thus to control by imposed power as in the Nazi state. Muller illustrates this in his own examples of the eminent men he might have chosen, for in his list of 1959 as compared with that of 1935, Lenin, Marx and Sun Yat Sen have been dropped and Einstein and Lincoln added.

To me such schemes seem to express the same sort of benevolent utopianism as did some of Galton's proposals of 60 years ago, but now they must be viewed in the light of some actual experience with them. Then as now they were backed by the prestige of men of deserved eminence in science, then of Galton, today of Muller, but this did not save the earlier programs from grave misuse and ultimate damage to both human society and science. In fact the high scientific standing of their proponents increases the dangers of uncritical acceptance of them as bases for social and political action, with the ever attendant risk of loss of public confidence in genetics as applied to man if or when their unsoundness becomes manifest.

Such considerations remind us of the dilemma which scientists face in their desire both to advance sound knowledge and to make it serve its essential social function. In the case of human genetics, I do not believe that the problems posed by the cohabitation of these two purposes are to be settled by divorce, as Bateson suggested. The problems posed by the continuing occurrence of diseases and defects ("Our load of mutations" Muller 1950) are real and they must be faced, both as biological and as social problems. Both sets of interests must be free to develop, and better together than separately for this is the condition under which common criteria for criticism and rigorous judgements, so badly needed in all fields affected by potential social applications, may be evolved.

If I have strayed somewhat from the limits of 1900-1930 that I had set for myself for a historical review, I suppose this is a reflection of my view that some of the cross currents operating in the earlier years of this century still play upon us. It seems to me that their influence in the first two or three decades was in part due to the lack of a clear vision of what studies of man have to contribute to the elucidation of general problems such as the mechanism of evolution and of gene action. The rise of population genetics and of physiological genetics have now turned attention to the rich source of material for these problems provided by human populations, and by the accumulated experience of medical and anthropological research. Recent discoveries, such as the identification of human genes controlling serological, biochemical, and developmental processes subject to the action of natural selection, should now give human genetics that orientation toward important biological problems which was not generally recognized in its early days. What seems to me to be most important, especially in its implications for the future, is the growing

recognition of the logical unity of genetics, for its essential problems, being concerned with a system of elements having similar attributes in all forms of life, can be seen to transcend the special problems of the different categories of organisms. Human genetics, freed from the narrower bounds and conflicting purposes which hindered its early growth, seems clearly destined to play an important role in the advancement of the whole science of genetics of which it is a part. And that, in the long run, may constitute its best contribution to the satisfaction of human needs.

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